SCIENCE.

FRIDAY, AUGUST 27, 1886.

COMMENT AND CRITICISM.

CAPT. C. E. DUTTON, of the U. S. geological survey, has been recently engaged in making a study of Crater Lake in Oregon, and the latest advices received from him show that he has discovered probably the deepest body of fresh water in the country. Leaving Ashland, Oregon, on the 7th of July, his party, escorted by ten soldiers provided through the courtesy of the general commanding the military department of the Columbia, reached the brink of the wall of the lake on the 13th, having brought with them boats so mounted on the running gear of wagons as to bear transportation over a hundred miles of mountain road without injury. The boats bore the transportation without strain or damage, and preparations were at once begun for lowering them nine hundred feet to the water. The steepness of the wall was very great, being at the place selected about 41° or 42°, and the descent partly over talus, above covered with snow, and rocky broken ledges lower down. The boats entered the water quite unharmed. The process of sheathing them, rigging the tackle, and lowering them occupied four days. A couple of days were occupied in making journeys around the walls of the lake by boat, — the only possible way, — and in examining the rocks and structures of the wall in its various parts. Next followed a series of soundings. The depth of the lake considerably exceeded the captain's anticipations, though the absence of any thing like a talus near the water line already indicated deep water around the entire shore. The depths range from 853 to 1,996 feet, so far as the soundings show, and it is quite possible and probable that depths both greater and shallower may be found. The average depth is about 1,490 feet. The descent from the water's edge is precipitous; at four or five hundred yards from shore, depths of fifteen to eighteen hundred feet are found all around the margin. The greatest depths will probably exceed two thousand feet, for it is not probable that the lowest point has been touched. The soundings already made indicate it as being the deepest body of fresh water in the country.

THE GREAT VALUE of chemical analysis in solving problems which are otherwise incapable of solution, was never better demonstrated than in the recent ice-cream poisoning which occurred in New Jersey. Various theories had been advanced to explain it, any one of which would have accounted for the symptoms produced in the sufferers. The tyrotoxicon discovered by Professor Vaughan, the vanilla bean used in the flavoring extract, and the gelatine employed to give stiffness, were credited with being the possible *materies morbi* by their respective advocates; but no one seems to have suspected foul play. The death of one of the victims was followed by a post-mortem examination, and the organs of the deceased were submitted to Professor Austen of Rutgers college. He has just announced the discovery of arsenic in sufficient quantity to cause death. It is more than probable, that, were the truth known, in all the cases of poisoning by food-products, malice would be found to play a more important part than either decomposition or germs introduced from without.

TWO PERSONS ARE REPORTED as having died from cholera at Chippewa Falls, Wis. That the cause of death in these cases was Asiatic cholera is in the highest degree improbable. Italy seems to be alone among European countries in having this scourge now prevalent, and that any one at so remote a point as the town mentioned should show symptoms of cholera, is hardly to be credited. It is, of course, possible that clothing infected with cholera might be carried in trunks, and opened at a point so far removed, and that those exposed might thus contract the disease, for there is good evidence to prove that infection has been thus conveyed; but it is more than probable that if cholera reaches this country, it will be from the seaboard. When the facts become known, it will doubtless be found, as has so often happened, that the cause of death in the cases at Chippewa Falls was a severe form of cholera morbus.

UNDER A RECENT ORDER of the treasury department, all restrictions on imported rags have been removed, and they have been placed upon the same footing as other merchandise; that is, to be

excluded, disinfected, or admitted according to the discretion of the local health officer. A great deal of unwarranted hostile criticism has been indulged in with reference to rag-disinfection in the port of New York. At a time when an epidemic of cholera was imminent, the health officer of that port consulted with the health officials of the neighboring cities, and of the state, and the action which he took in reference to the disinfection of rags was based upon that conference, and has received the endorsement of the sanitary authorities of the country. While New York has from the very first been vigilant, other cities have been careless and negligent; and that contagious disease has not been introduced by means of infected rags, is due to good luck rather than to good management. That there should be some federal control of such matters goes without saying, for, while state rights are to be respected, there is such a thing as carrying that principle too far. The right to permit contagion to enter and ravage the country, because a quarantine would be expensive, is not a right which any state can claim as guaranteed it by the constitution. If the general government can restrict the sale of oleomargarine, it can certainly be no great stretch of its powers to adopt such general measures as will apply to all its ports of entry, by which commerce and the public health are at the same time protected.

DR. HARRINGTON, OF BOSTON, has recently had under his care four patients suffering from chromium poisoning. The first case was that of a cap-maker, who, after handling and cutting a large quantity of dark-blue cloth for the manufacture of military caps, began to suffer from an intolerable itching of the hands, face, neck, and scalp, which was followed by ulceration, causing running sores. The symptoms disappeared after she ceased work upon this cloth, and returned when she renewed her work upon it. The second case was that of a clergyman, who was similarly affected after wearing a pair of brown woollen gloves. The other cases were young children, who had, previous to the appearance of the first symptoms, put on for the first time new suits of brown woollen clothes. An analysis of the goods in all the four instances revealed chromium. The chromium mordants are now being extensively employed in dyeing, much more so than formerly, and the range of colors produced by their aid is very great, including brown, brownish red, claret red, olive, yellow, old gold, purple, blue, black, buff, and gray. Dr. Harrington, at the conclusion of his paper describing these cases, read before the Massachusetts medical society, says that it is yet to be determined whether in these cases the compounds formed by the mordant and the dye-stuffs are in themselves the active poison, or are decomposed by the secretions of the body, with liberation of simple chrome compounds.

THE BUFFALO MEETING.

THE least that can be said of the meeting of the American association for the advancement of science which has just closed, is that it was thoroughly enjoyable. The arrangements made by the local committee for the entertainment of the association were admirably adapted to promote the objects of the meeting. The simple habits of the members led them to welcome rather than to regret the absence of official festivities on a large scale, but prepared them to enjoy the hospitalities tendered by leading citizens and organizations, which were noteworthy both for their ample scale and their unostentatious simplicity. On the excursions to Grand Island and to Niagara, every opportunity for pleasure and profit was afforded without in any way troubling the members by detailed programmes or burdensome attentions.

The smallness of the meeting was its only disappointing feature. The beautiful summer climate of Buffalo, its central position between the east and the west, and the prospect of a visit to one of the grandest and most interesting of natural phenomena, just freed from the onerous exactions which such a visit used to entail, would, it was expected, attract one of the largest assemblages of members that had yet been witnessed. Yet, not one-fourth of the membership was found at the meeting. The paucity of southern members was especially noteworthy. One great purpose of the organization is to bring into contact the intellectual element of the north and the south as well as of the east and the west, and the association can render no more worthy service than that of promoting education as well as research in every quarter of our land. It is much to be desired that workers and educators in the south should point out to their colleagues in the north how that stimulus of personal contact, sympathy, and attention, so necessary to the fulness of intellectual development, can best be secured to their section.

The scientific outcome of the meeting is, on the

whole, encouraging. The writers of popular essays were out in rather less force than usual. We did not notice on the programme the title of a single paper attacking the theory of gravitation. Communications of doubtful value appeared in about the usual proportion. Statements of careful observations, and well-matured results appeared in larger proportion than usual, yet, there was no announcement of a brilliant discovery, or of a research of extraordinary importance. All that can certainly be noticed is a well-marked tendency to improvement. Notwithstanding this improvement, the question is still open whether the association can reach the highest standard of usefulness by aiming to be primarily a medium for the communication and publication of scientific papers.

That the system in vogue at present is not satisfactory in all points must be conceded by all. A member visits the place of meeting in the morning, and receives a programme for the day, showing what papers are to be read before each section. He finds two or three that he wants to hear, and two or three more that he would like to know something about in order to decide whether he does or does not want to hear them. But the only way to learn anything about one class or the other is to wait patiently till they are called in their turn. There are perhaps two or three papers to precede any in which he is interested. He waits for one, because the author has estimated its length at only ten minutes. But the author occupies twenty minutes with details so prolix and tedious that his hearers are weary when he gets through. Then the presiding officer calls for remarks. No one is ready to proffer any remark, and the next paper is about to be called when some one, out of pure charity, drops a remark. Another replies, and very soon a desultory debate is in progress having little relation to the subject of the paper. Our hearer estimates that an hour will be required to reach the paper he wants to hear, and leaves the room. In order to be sure he returns in half an hour, to find that the authors of the intermediate papers were absent, and that, in consequence, the paper he wanted to hear has already been read. He has thus spent an hour without any profitable result whatever.

The system which leads to such results calls loudly for improvement. Specified hours should be assigned for hearing and discussing specified papers. Debate upon subjects of interest suggested by any communication should be allowed for in advance. Less formality in the presentation of papers should be observed. There is no necessity of entering into the long details with which members so often weary their hearers, who would be satisfied to hear the pith of a communication. It will also be well for members to consider whether the conception of the association as a body organized solely for the reception and publication of original researches might not well be modified. Scientific societies meeting at short intervals are now so numerous that a body which assembles only once a year is at a great disadvantage as a medium of publication.

On the other hand, the social feature of the meetings should be more clearly recognized. No class of men are so much in need of contact with their fellow-workers as those who are exploring the fields of science, and in no other enlightened country is this contact so difficult as in ours. As matters now stand, we believe that the association can do more good by bringing men together to talk over the work of the year, and the prospects of the future, than by remaining a medium for receiving original communications. To do this effectively requires a common understanding among the older members, in virtue of which more of them will be in attendance. This again requires extensive, though not very radical changes in the method of procedure adopted by the standing committee, the several sections, and the association at large. The system which we think should be aimed at is one in which the exposition, by leading members, of their work during the year, whether published or unpublished, shall be a prominent and well understood feature. In a word, everything that can be done to make the meetings attractive and profitable will add in an increasing ratio to the success of the organization. And the most urgent requirement is a plan by which every member shall be able to hear what he wants to without being required to listen to anything he does not want to hear. Such a plan will react upon the members by supplying an incentive to the preparation of communications by those members who might say something of special interest to their fellows, but who are now deterred from so doing by the absence of the proper arrangements for being heard.

Withal, there is the constant necessity of such familiarity on the part of those who are to read papers with what they have to say, and how they intend to present it, that they may not bore their audiences with trifling details.

MULTIPLE ALGEBRA.

Professor Gibbs's masterly address upon the subject of 'multiple algebra' was too long and of too technical a nature for presentation in full to our readers, and the quotation of a few passages, and a brief summary of the bearings of the remainder, must suffice to acquaint them with its general drift and importance. His opening remarks were as follows: —

"It has been said that 'the human mind has never invented a labor-saving machine equal to algebra.' If this be true, it is but natural and proper that an age like our own, characterized by the multiplication of labor-saving machinery, should be distinguished by an unexampled development of this most refined and most beautiful of machines. That such has been the case, no one will question. The improvement has been in every part. Even to enumerate the principal lines of advance, would be a task for any one, — for me, an impossibility. But if we should ask in what direction the advance has been made, what is to characterize the development of algebra in our day, we may, I think, point to that broadening of its fields and methods which gives us 'multiple algebra.' Of the importance of this change in the conception of the office of algebra, it is hardly necessary to speak: that it is really characteristic of our time, will be most evident if we go back some two or three score years, to the time when the seeds were sown which are now yielding so abundant a harvest. The failure of Möbius, Hamilton, Grassmann, Saint-Venant, to make an immediate impression upon the course of mathematical thought in any way commensurate with the importance of their discoveries, is the most conspicuous evidence that the times were not ripe for the methods which they sought to introduce. A satisfactory theory of the imaginary quantities of ordinary algebra, which is essentially a simple case of multiple algebra, with difficulty obtained recognition in the first third of this century. We must observe that this 'double algebra,' as it has been called, was not sought for or invented, — it forced itself, unbidden, upon the attention of mathematicians, and with its rules already formed."

The speaker then gave a critical historical review of the different contributions of Hamilton, Möbius, Grassmann, Saint-Venant, Cauchy, Cayley, Hankel, the Peirces, father and son, and Sylvester, to these new methods of mathematical analysis, showing the additions and developments made by each to the various subjects.

In the second part of the paper, Professor Gibbs criticised the methods of some modern writers on these subjects, showing how they failed to grasp the full significance and bearings of the matters they were dealing with, being too much hampered by the old ideas and methods of simple algebra. We quote here a few sentences: —

"This fault has been denounced by Sylvester; and if any one thinks that I make too much of the stand-point from which we view the subject, I will refer him to the opening paragraphs of the lectures on 'universal algebra' in the sixth volume of the *American journal of mathematics*, where, with a wealth of illustration and an energy of diction which I cannot emulate, the most eloquent of mathematicians expresses his sense of the importance of the substitution of the idea of the matrix for that of the determinant. If this is so important, why was the idea of the matrix let slip? Of course, the writers on this subject had it to commence with. One cannot even define a determinant without the idea of a matrix. The simple fact is, that the writers on this subject have especially developed those ideas which are naturally expressed in simple algebra, and have postponed, or slurred over, or omitted altogether, those ideas which find their natural expression in multiple algebra. But in this subject, the latter happened to be the fundamental ideas, and those which ought to direct the whole course of thought." Many illustrations were then given of the applications of these ideas to cases in point.

The author introduced the third part of his paper as follows: "We have considered the subject a good while from the outside; we have glanced at the principal events in the history of multiple algebra; we have seen how the course of modern thought seems to demand its aid, how it is actually leaning toward it, and beginning to adopt its methods. It may be worth while to direct our attention more critically to multiple algebra itself, and to inquire into its essential character and its most important principles. I do not know that anything useful or interesting, which relates to multiple quantity, and can be symbolically expressed, falls outside the domain of multiple algebra. But if it is asked, what notions are to be regarded as fundamental? we must answer, here as elsewhere, those which are most simple and fruitful. Unquestionably, no relations are more so than those which are known by the names of addition and multiplication."

Then followed a long discussion of the fundamental conceptions and methods of modern math-

Abstract of an address before the section of mathematics and astronomy of the American association for the advancement of science at Buffalo, Aug. 19, 1886, by Prof. J. Willard Gibbs, of New Haven, Conn., vice-president of the section.

ematics, which nothing but publication in full could render intelligible, and that only to the mathematicians among our readers. To such, its full publication in the 'Proceedings' will prove of the greatest value.

The fourth part of the paper was devoted to consideration of some of the applications of multiple algebra. From this we quote the following: "First of all, geometry, and the geometrical sciences which treat of things having position in space, — kinematics, mechanics, astronomy, crystallography, — seem to demand a method of this kind, for position in space is essentially a multiple quantity, and can only be represented by simple quantities in an arbitrary and cumbersome manner. For this reason, and because our spatial intuitions are more developed than those of any other class of mathematical relations, these subjects are especially adapted to introduce the student to the methods of multiple algebra. Here nature herself takes us by the hand, and leads us along by easy steps, as a mother teaches her child to walk. In the contemplation of these subjects, Möbius, Hamilton, and Grassmann formed their algebras, although the philosophical mind of the last was not satisfied until he had produced a system unfettered by any spatial relations. It is probably in connection with these subjects that the notions of multiple algebra are most widely disseminated. Maxwell's 'Treatise on electricity and magnetism' has done so much to familiarize students of physics with quaternion notations, that it seems impossible that this subject should ever again be entirely divorced from the methods of multiple algebra. I wish that I could say as much of astronomy. It is, I think, to be regretted, that the oldest of the scientific applications of mathematics, the most dignified, the most conservative, should keep so far aloof from the youngest of mathematical methods; and standing, as I do to-day, by some chance, among astronomers, although not of the guild, I cannot but endeavor to improve the opportunity by expressing my conviction of the advantages which astronomers might gain by employing some of the methods of multiple algebra. A very few of the fundamental notions of a vector analysis, the addition of vectors and what quaternionists would call 'the scalar part and the vector part of the product of two vectors' (which may be defined without the definition of the quaternion), — these three notions, with some four fundamental properties relating to them, are sufficient to reduce enormously the labor of mastering such subjects as the elementary theory of orbits, the determination of an orbit from three observations, the differential equations which are used in determining the best orbit from an indefinite number of observations by the method of least squares, or those which give the perturbations when the elements are treated as variable. In all these subjects, the analytical work is greatly simplified, and it is far easier to get the best form for numerical calculation than in the use of the ordinary analysis."

Then followed illustrations of the various methods of applying multiple algebra to different classes of problems, and the paper closed as follows: "But I do not so much desire to call your attention to the diversity of the applications of multiple algebra, as to the simplicity and unity of its principles. The student of multiple algebra suddenly finds himself freed from various restrictions to which he has been accustomed. To many, doubtless, this liberty seems like an invitation to license. Here is a boundless field in which caprice may riot. It is not strange if some look with distrust for the result of such an experiment. But the further we advance, the more evident it becomes that this, too, is a realm subject to law. The more we study the subject, the more we find all that is most useful and beautiful attaching itself to a few central principles. We begin by studying 'multiple algebras;' we end, I think, by studying 'multiple algebra.'"

SEAT OF THE ELECTROMOTIVE FORCE.

Professor Brackett's address was essentially a résumé of the history of the investigations to find the source of the current in galvanic batteries. No attempt was made to settle the question, which has been so long a bone of contention.

The address was so purely historical in its nature, and, withal, was so condensed and concise, that any abstract would be necessarily little more than an index of its contents. Those who are interested in the subject must await its publication in full in the 'Proceedings' of the association.

Galvani's two accidental discoveries were made in 1789: the one was the influence of an electrical machine in causing contractions in a frog's legs, and the other the production of sufficient electricity to cause the contraction by touching two joined strips of copper and zinc to the moist animal tissues. Naturally from these results there arose a theory of the identity of nerve-force and electricity, — the so-called animal variety of electricity. While this controversy, soon to subside, was started among physiologists, a much more

Abstract of an address delivered before the section of physics of the American association for the advancement of science at Buffalo, Aug. 19, 1886, by Prof. C. F. Brackett, of Princeton, vice-president of the section.

violent one has continued to rage among physicists. Is the electricity of the galvanic cell due to chemical action or to contact of dissimilar substances? It is to the history of the attempts to answer this question that the address is devoted.

PROGRESS OF MECHANICAL SCIENCE.

THE recent enlargement of the scope of this section to include all branches of engineering, and the increasing interest manifested in its meetings, warrant my making some remarks as to the true objects of the section, and the means of increasing its usefulness in the future.

In marked contrast with the past, the present age is one of pronounced material development. Formerly the brightest and most gifted men devoted themselves to religion, philosophy, politics, exploration, art; but for the past hundred years the attention of the leading men of the civilized world has been directed to increasing and cheapening those products which minister to the daily life and comfort of man. Farmers, mechanics, and laborers live now more comfortably than did the middle classes of feudal times; the duration of human life has been materially lengthened, and all portions of society recognize the importance of further progress, and the advantage of organization and invention in securing it.

This era of material progress may be said to have commenced with the final perfecting of the steam-engine, which, together with the various attendant machines, takes the place of hand and animal labor, and which has increased and cheapened the production of the necessaries and luxuries of life; and it has pushed the inventor and the engineer to the front rank in modern society. It may be useful to point out the absolute necessity of verbal and written intercourse between investigators and inventors, that the speculation and curiosity of the former may ripen into the effective invention of the latter. Nothing is more remarkable than the multitude of minds and facts which are required for the perfecting of even a simple machine, nor how little the last man may need to add to complete the invention. Facts and natural laws, known for years as curiosities, are taken up by some inventor, who fails in the attempt to render them of practical use; then a second genius lays hold, and, profiting by the mistakes of the first, produces, at great cost, a working machine. Then comes the successful man, who works out the final practical design, and, whether making or losing a fortune, he yet permanently benefits mankind.

The faculties of invention and discovery are generally separate. One set of men observe facts, and deduce laws therefrom; and another set endeavor to turn the results of this observation and deduction to practical account in the production of labor-saving appliances. This section should be the place where these men may meet one another, and profit by the interchange of ideas. Many of the men whom I see before me are devoting their lives to the study of nature, with no desire to make money out of it, but simply to increase human knowledge; and some of their discoveries will eventually be put into practical shapes for the use and convenience of man. History proves, too, that the scientific observers have the safer and happier part. Their success may not be so dazzling as that of some great inventors, but they do not have to bear such bitter trials and disappointments. To deduce natural laws requires mental accuracy in observing and reasoning; to make them useful in doing the world's work requires imagination and ingenuity. Sometimes long years must pass, and generation after generation of inventors wear their lives out, before a needed machine becomes an accomplished success. Evidently, then, the greater the number of minds that can be brought to bear upon a particular problem, the greater is the chance of early success. I believe that it is the particular province of this section of the association to bring these two classes of minds together, and to promote their intercourse, that the discoverer may learn in what direction fresh information is needed, and that the inventor may be advised as to what is already known.

The well-worn history of the steam-engine gives us an instance of an invention which did not spring full-grown from the brain of the inventor. History informs us that it commenced to exist two thousand years ago, in the eolipile of Hero of Alexandria. His treatise remained hidden until translated and printed in 1547; and then Branca, the Italian architect, constructed one for pounding drugs. Hero's book ran through eight editions in different languages, and attracted the attention of a French inventor, who tried vainly to raise water by steam pressure. Then came the Marquis of Worcester, who died a disappointed man after spending $250,000. Then de Morland tried using steam in cylinders, instead of in contact with the water; Papin built a steamboat, only to have it seized and destroyed while on its way to England, and he, too, died broken-hearted and poor; Savery went back to using the steam directly in contact with water; and finally Newcomen

Abstract of an address before the section of mechanical science of the American association for the advancement of science at Buffalo, Aug. 19, 1886, by O. Chanute, Esq., Kansas City, vice-president of the section.

built an engine that worked; and between 1705 and 1758 quite a number were erected. These engines had a duty of only 5,500,000 foot-pounds per pound of coal, the improvements of James Watt, an instrument maker, increasing the duty to 60,000,000.

My object in giving this sketch is to call your attention, first, to the gradual evolution of an invention by the process of exclusion, by finding out what would not do; and second, the apparent chain of connection, running for over a century, through several generations of inventors, each evidently profiting by the failures of his predecessors, to the extent, at least, of avoiding their repetition. Is it not evident that the earlier inventors would have accomplished greater results had they had a larger range of scientific experiments and advice; and that Watt triumphed because he had the whole faculty of the University of Glasgow at his back, to give him knowledge of natural principles, and information as to what had been done? So with other inventions; the steamboat was being developed from 1760 to 1807; the locomotive, from 1802 to 1829; the telegraph from 1729 to 1844; the sewing machine, with its two thousand patents, from 1790 to 1860; the reaping machine, for seventy-five years, and so on, — the last successful man adding generally but little to what had been done before. The rule is, that "the basis of success lay in a thorough acquaintance with what had been done before, and in setting about improvement in a thoroughly scientific way."

My own observation has acquainted me with the development of the ice-making machine. The economical production of cold by the combustion of fuel was a matter of theory when, in 1755, Professor Cullen experimented in Glasgow with 'quick-lime and spirits of sal-ammoniac' as the best volatile substance for producing cold. His discoveries remained as laboratory experiments until Jacob Perkins, in 1834, obtained a partial success in producing ice by the evaporation of ether. Then came Professor Twining, of New Haven, Leslie, Valance, Harrison, Pontifex Seibe, Windhausen, Tellier, Carré, and Pictet, with more or less doubtful success. Up to 1869, the machine was in the experimental or unsuccessful stage. Then came an experimenter who deliberately read up the whole subject in a library, and made himself master of what patent-attorneys call 'the state of the art,' and of the scientific principles concerned, working, according to his own account, 'harder than he ever had before in his life.' He discarded the usual working fluids, and adopted anhydrous ammonia. After various struggles and successes, the machine was adapted to the difficulties of the case, and put in successful operation in 1874, since which time it has become of immense practical importance in warm climates, for making ice, cooling breweries, etc., though giving an efficiency of but seventy per cent. In 1877, another inventor set himself deliberately to improve the machine. He put a practical mechanic, a chemist, and a patent-attorney to work, and in 1878 built a machine, which, however, gave no improved results. He did not let the matter rest here, however, but persevered, and in 1880 built an entirely successful machine, which did the work for which seven thousand tons of ice had been required. So rapid has been the introduction of refrigerating machines, that there are now several hundred of various makes at work in the United States. They produce as much cold for each ton of coal consumed as would be obtained by the melting of twenty tons of ice, at which rate natural ice is worth only seventy-five to eighty cents per ton, or less than the usual cost of harvesting and storing it.

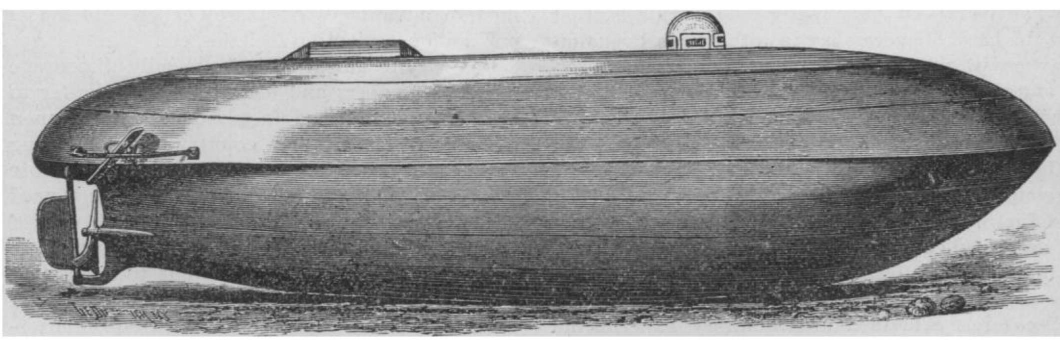
In comparing this development with that of the steam-engine, we see the difference between the scientific way of working out an invention and the former disjointed way, when each man had to rely chiefly upon his own experiments; and also the difference between ancient facilities and the modern advantages offered by experts, technical publications, scientific societies, etc.

Ordinary technical societies usually discourage speculative papers and discussions, and prefer to hear of accomplished facts; but the busy men who are developing this country need something more,— they need to keep up with discovery before it is reduced to practical account, and they need that personal contact and sympathy with men of science which nothing can replace. Engineers, as well as other practical men, owe it to themselves to come to these meetings, bringing accounts of what they have done and hope to do, and especially of what they have failed to do, and why; and some speculative papers may well be allowed providing always that they are on a sound basis, and stick to facts; for how often is it that the imagined things of to-day become the accomplished results of to-morrow!

To encourage good work in the preparation of papers, might there not be established, by friends of the association and section, prizes for the best papers on a number of important subjects? I hope to see something done in this direction before the close of the meeting. I hope also to see the practice inaugurated for members, during the year or meeting, to propound queries upon subjects about which they wish information or discussion. I should like also to see published annu-

ally lists of subjects upon which papers are desired by the section, as was done to some extent in the recent circulars of the section. In this way, live subjects are apt to be most beneficially canvassed, and experiment and discovery kept in the right paths. It may be well, in this connection, to mention some inventions which are now, so to speak, 'in the air;' of course, we all recognize that the flying-machine belongs to this class, in one sense if not in another, and a paper upon it has been presented which may prove of interest to you. What is needed, however, is a sufficiently light motor, without which a flying-machine cannot be expected to succeed. Steam power, also, for agricultural work in its many forms, is not yet an accomplished fact; and we may mention one machine greatly needed, — a cotton-picker. Then, too, there is the electric motor for street traffic, which needs further improvement; also the transmission of power over great distances, electric lighting, etc., etc.

But I have said enough to indicate how large a field may, in my judgment, be covered by this section of mechanical science and engineering, and how its meetings may in the future be made still more useful and interesting than they have been in the past.



TUCK'S SUBMARINE TORPEDO BOAT.

A NEW SUBMARINE TORPEDO BOAT.

THE accompanying illustration represents a new submarine boat, invented by Mr. J. H. L. Tuck, and now being tested in this city, with highly satisfactory results. On Tuesday, August 24, the first public exhibition of the vessel was given in the Hudson River, opposite 86th street, in the presence of a number of scientific men. Manned by a crew of two men, pilot and engineer, she started off at a good rate of speed, disappeared, travelled perhaps half a mile without making a ripple to indicate her whereabouts, and reappeared at the pleasure of the pilot. During the two hours' test, she travelled several miles, answering her helm as readily as a steam yacht. The boat is an iron spindle thirty feet long by eight in diameter, with a propeller, and vertical and horizontal rudders. The motive power is a fourteen horse-power Westinghouse engine, furnished with steam from a caustic-potash reservoir, which is charged from an outside source. Deadlights in the conning-dome forward, together with a compass, enable the pilot to shape his course. Ingress and egress are effected through an aperture in the hatchway near the stern, which may be hermetically sealed from the inside.

NOTES AND NEWS.

THE officers for the next meeting of the American association are as follows: President, S. P. Langley. Vice-presidents: mathematics and astronomy. Wm. Ferrel; physics, Wm. A. Anthony; chemistry, Albert B. Prescott; mechanical science and engineering, Eckley B. Coxe; geology and geography, G. K. Gilbert; biology, W. G. Farlow; anthropology, D. G. Brinton; economic science and statistics, Henry E. Alvord. Permanent secretary, F. W. Putnam; general secretary, W. H. Pettee; assistant general secretary, J. C. Arthur. Secretaries of the sections: mathematics and astronomy, Henry M. Paul; physics, C. Leo Mees; chemistry, C. F. Mabery; mechanical science and engineering, Geo. M. Bond; geology and geography, T. B. Comstock; biology, J. Henry Comstock; anthropology, F. W. Langdon; economic science and statistics, Wm. R. Lazenby. Treasurer, William Lilly.

— Twenty drops of bromine in an ounce of olive-oil, applied freely four times a day, and the affected part washed with warm water and castile soap twice a day, is said to have completely cured seventy-five cases of ivy poisoning.

— The Brazilian government has appointed a commission of physicians, from Rio, Bahia, Maranhao, and Pará, to study the *beri-beri*. This disease is particularly prevalent through the littoral provinces of the north, and has been gradually gaining ground of late.

— An examination, by an oculist, of the eyes of one thousand one hundred persons who work by the incandescent electric light, fails to show any injurious effects produced by that light. The arc-light may cause eye-trouble if in too close proximity.

— That human hair retains its characteristics for long periods of time, and, indeed, is well-nigh indestructible, is a fact of common observation. A remarkable instance of this is found in a wig which has recently been discovered in an Egyptian temple at Thebes, and is now deposited in the British museum. It is supposed to have been part of the attire of an Egyptain priest, and from the circumstances of its discovery is regarded as being at least 3,400 years old.

— Food given when cold is more likely to be retained by a sensitive stomach than in any other condition, and ice will not be rejected when all other substances are thrown off; acting upon this fact, frozen milk is now given in cases of sickness attended with irritable stomach, especially in fevers.

— London consumes daily two million eggs, and the rest of England an equal number. Of these one-half comes from Italy via the St. Gothard tunnel, and the others are brought from Denmark, Germany, Belgium, and France.

— At the Michigan state sanitary convention, to be held at Big Rapids in November next, the following topics will be discussed: The hygiene of schools; Pasteur and protective medicine; public-health laws; alcoholic drinks, — are they foods or are they poisons? the injuries of every-day drug-taking; what to eat, when, and how; and, the prevention of communicable diseases.

— *La graphologie*, a French journal, describes a new method of reading character, known as 'scarpalogy.' It consists in a study of the heels and soles of shoes. If these are worn down evenly, the wearer is a good business man, energetic and quick in decision; if the outer side is worn more than the inner, he is of an adventurous turn of mind. Weakness of character is indicated by a heel and sole worn most on the inner side.

— Prof. C. L. Ford, of the L. I. college hospital, called attention, in 1862, to the fact that the lower limbs were not always of the same length in the human subject. This statement induced anatomists and surgeons to make an extended series of measurments, and the results confirm the opinion expressed by Professor Ford. Garson, in the *Journal of anatomy and physiology*, sums up these observations. In seventy skeletons examined, he found the lower limbs equal in but seven. His measurements show that in 54.3 per cent the left limb was longer than the right; in 58.5 per cent the left thigh-bone was longer than the right. The right tibia was longer than the left in 41.4 per cent, and the two bones were found equal in but 10 per cent. The difference in the length of the lower limbs varies from one-eight of an inch to one inch and five-eights, without any deformity being recognizable. In a series of measurements of the collar-bones, only six, in twenty-two cases examined, were found to be equal.

— The scientific writings of Henry James Clark have received careful attention from Prof. Fred. Tuckerman, in his biographical notice for the catalogue of the Massachussetts agricultural college (1886). The bibliographical list contains twenty-six titles, — three new ones being added to the national academy list. Professor Tuckerman has also improved the national academy list by references to numerous English reprints of Professor Clark's papers. British students will find these of use. The third title, of some interest in the history of American zoölogy, reads as follows: 'Contributions to the natural history of the United States, 1857–62 (conjointly with Prof. Louis Agassiz).'

— The practice of medicine in Russia is exceedingly onerous and unremunerative. A physician who fails to respond to the summons of a patient is punished by a fine of from five to one hundred roubles. If the case was a dangerous one, and the physician knew it, he may be imprisoned in the jail for three months. The legal fee for an ordinary visit is from seven and a half to fifteen cents; for an accouchement, seventy-five cents. These laws are strictly enforced. An elderly German physician, an invalid, was called, on a stormy winter night, to attend a case seven miles distant. He objected to go unless he was reasonably remunerated, naming his fee. The messenger left to ascertain whether this amount would be paid, but did not return. The physician was subsequently arrested, tried, and condemned to eight days imprisonment. Besides, he had to pay his lawyer two hundred and fifty dollars in advance.

— A few weeks since, some members of the Davenport (Iowa) academy of sciences explored

several mounds in Louisa county in that state. In the one most thoroughly examined, from near the surface to the bottom, were found decayed logs of from six to ten inches diameter, lying in irregular positions, not charred, but lying in and covered by a thin layer of ashes. In this mound was also discovered a skull, and near it several relics. Under the skull was found a copper axe, entirely covered with cloth, and wrapped in bark, well preserved. Four other copper axes were also found, all showing they had been wrapped in cloth. Near these were three copper awls, one found lying and two sticking upright in the floor of the mound. Also a quantity of shell beads and sheets of mica. Two curved-base pipes were also found near the head, one of ordinary gray pipe-stone, bearing a well-carved figure of a hawk, with pearl eyes; the other was a plain round bowl, but is unique in material, being made of calcite, beautiful in form, and quite translucent. Several other mounds were partly explored, but nothing of special interest was found in them.

— *Il popolo Pisano*, an Italian journal, claims that Pasteur's method of preventing rabies by inoculation with virus, was practised in Italy by Dr. Eusebio Valli as long ago as 1799. He employed for this purpose the saliva of a rabid dog, mixed with gastric juice. Having ascertained by experiments upon the lower animals that this method of treatment was a safe one, he inoculated two residents of Pisa with the same material. Although these persons — one a child, the other an adult — had been bitten by a mad dog, rabies did not develop in them.

— Bichloride of mercury, commonly known as corrosive sublimate, has of late years been largely used by the medical profession in a state of solution, whenever, in the treatment of wounds, antiseptic or germicidal agents were desirable. This same substance is now extensively employed as a germicide in the purification of articles and places which are infected with disease of a zymotic nature. Statistics have recently been collected which tend to prove, that in the strength usually employed, 1 part of the bichloride to 1,000 parts of water, the use of this solution is not without danger when brought in contact with any portion of the surface of the body from which the skin or the mucous membrane has been removed, as, for instance, in open wounds. There have been reported and verified thirty deaths which are attributable to the use of this solution in the strength mentioned. It is, however, when properly employed, not only a very valuable, but also a perfectly safe agent; and it will doubtless be found, that, as an irrigant to exposed surfaces of the human body, more dilute solutions will accomplish all that is desired. Where it is used as a disinfectant for articles of clothing or furniture, no danger can possibly accrue from its use, even in the proportion of 1 to 1,000.

— The great advantage, from a hygienic point, of the electric light over gas in halls where large numbers assemble, is well shown by the elaborate researches of Dr. Breslauer, and recorded in the *Deutsche medicinische zeitung*. The experiments were made in the Munich theatre, and included an inquiry into the air of the different portions of the house as to temperature and the products of combustion. The temperature was increased in the parquet ten times more with gas than with the electric light, and three times more in the gallery. The amount of carbonic acid per 1,000 was, —

	Electric light.	Gas.
In parquet	0.055	3.926
In gallery	0.870	3.151
In centre of gallery	1 178	4.353

This increase in the amount of carbonic acid is one of the principal disadvantages of gas as compared with the electric light. The conclusions which are drawn from these observations are that the air remains much purer and at a lower temperature in all parts of the house, especially in the galleries, when electric light is employed as a means of illumination.

— Turkey has a medical school at Constantinople, at which there are annually more than three hundred students, of which number some sixty graduate. Each course continues during nine months of the year, and six years must be spent in medical study before a diploma can be received. Instruction is given in the Turkish language, as most of the students are Turks.

— Constantinople has at the present time a water-supply from Lake Dercos, twenty miles from the city. This was introduced by a French company, and was intended to supplant or supplement the supply, which the city has had for years, from an open reservoir six miles distant, in which the rain collected, and from which it was brought in iron pipes.

— From the *Medical and surgical reporter* we learn that the willow is now being largely cultivated in America for medicinal purposes. On one farm in Georgia there are 400,000 willows grow-

ng, and 80,000 additional slips have recently been put in. At the end of two years the switches are cut and made into bunches like sheaves of wheat. The leaves and the bark contain the medicinal salicin. This substance crystallizes in plates, is white in color, not very soluble in water, and somewhat bitter to the taste. Like other bitters, it promotes the appetite, and aids digestion, and is regarded as an excellent tonic in some forms of dyspepsia. It is also largely used in the treatment of acute rheumatism, and to some extent in malarial fevers as a substitute for quinine. It is said that the willow switches, when dry, are worth two hundred dollars a ton. The leaves and bark are sold at the rate of twenty-five cents a pound.

— A young woman is said by a writer in the *Medical and surgical reporter* to have acquired the habit of eating roasted coffee, eating sometimes as much as half a pound a day, and continuing it for four months. She was very pale, sallow, and nervous; she had a weak pulse, impaired digestion, and got out of breath easily going up stairs.

— Professor Bystroff has recently examined 7,478 children in the schools of St. Petersburg, and finds that 11.6 per cent suffer from headache. He regards it as due to irritability of the brain, brought on by the excessive forcing of the education.

— The entire population of Germany, as enumerated in the quinquennial census of December last, is given at 46,840,587, an increase of 1,606,526 over that of 1880.

— The entire length of railroads of the world, up to the end of 1884, as recently published by the Prussian minister of public works, was 291,000 miles, an increase of twenty-seven per cent, or over sixty thousand miles, during the preceding five years. Of the entire length, very nearly one-half is that of the American railroads, mainly in the United States.

LETTERS TO THE EDITOR.

****Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

A contribution to the psychology of the polar bear (Ursus maritimus Lin.).

The fact that bears occasionally create rotary currents in water by means of their paws, for the purpose of bringing floating objects within their reach, has several times been verified by different observers (Romanes' 'Animal intelligence,' New York, 1883, pp. 351, 352; Darwin's 'Descent of man,' New York, 1875, p. 76). Still, this act of bears has not been so often recorded as to render the present instance uninteresting.

In May, 1886, happening to be in Central park, New York city, I visited the bear-pit. This pit is divided through the centre by a partition of iron bars, black bears being confined on one side and two polar bears on the other. The water supply is furnished by an oblong basin about eight feet long by four or five wide, so placed that the above mentioned partition runs through its short axis. One of the polar bears was resting on the side of the basin, opposite to the front of the pit, with the side of his head snug against the partition, the body being stretched out alongside the margin of the basin, and his fore-paws hanging over its edge. In his fore-paws he had a portion of an ordinary walking-cane, about a foot and a half long, and from this he evidently derived, by playing with it in the water, a great deal of enjoyment. Let this bear be known as No. 1. The other bear, not being able to reach across the basin, nor to reach over the head to the fore paws of No. 1, and having no plaything of his own, was apparently highly discomfited. This discomfiture he manifested by his quick and uncertain turns around the pit, ever returning to the edge of the basin or the back of No. 1, there to again make an unsuccessful attempt to obtain the cane. Let this bear be known as No. 2.

An interested group of spectators had now collected, and one of them, out of sympathy for No. 2, threw him a small painted stick about eight inches long. This No. 2 immediately began playing with, taking it in his mouth and tossing it around in various directions. Finally the little bit of wood fell into the basin of water within reach of No. 1, who hastily appropriated it, much to the seeming chagrin of No. 2, — this bear once more becoming very restless and uneasy. The stick and the cane, however, were too much for No. 1 to manage, for in his maneuvres, seeming unnoticed of him, he lost his hold upon the stick, and it fell into the water. At once No. 2, who, at the time, was sitting on his haunches at the front side of the basin, appeared to comprehend this, and began pawing the water with the right and left paw alternately, thus creating a current in the water which brought the little piece of wood to him from the other side of the basin in about two minutes. Hastily taking it out of the water, No. 2 laid it on the edge of the basin, for in the mean time No. 1 had changed his hold on the cane in such a way that he confined it between his paw and the side of the basin at the water's level. His paw being at the very remote end of the cane, thrust partly through the bars into the black bear's side of the pit, the cane seemed to be free upon the water. No. 2 now went through the same motions as had secured him the small piece of wood. After keeping his exertions up for about one half-minute, he seemed to perceive that the cane did not move towards him: so careening his head around, he brought into full view the paw of No. 1, and appeared to comprehend that the cane passed between the paw of No. 1 and the basin wall, for instantly he stopped pawing the water, and went to playing with his own piece of wood.

Throughout this whole scene, it seemed to me that there occurred a notable change in the facial expression of each bear as he gained or lost a point. There was no evidence of anger; and while No. 2 was creating the water current, his face wore the impress of the most profound earnestness, which gave way, when the stick was obtained, to an expression of great elation, this in turn being replaced by an in-

tense look of cunning as he set about to obtain the cane.

First agreeing that the terms 'reflex action,' 'instinct,' and 'reason' shall be defined according to the definitions of Dr. Romanes ('Animal intelligence,' p. 17), the action, or series of actions, executed by No. 2 must have been, wholly or in part, either reflex, instinctive, or rational. If reflex, there must have been : —

(*a*) Particular and often recurring stimuli, to have given rise to the acts of No. 2 ; and also,

(*b*) The acts must have been adaptive, although not intentional.

Manifestly, these two prime conditions did not obtain, and therefore the acts of No. 2 were not reflex, either in whole or in part. If instinctive, then the acts of No. 2 must have been performed "without necessary knowledge of the relation between means employed and ends attained, but similarly performed under similar and frequently recurring circumstances by all the individuals of the same species."

1° Had No. 2 ceased his current-making when he obtained the piece of wood, his act might possibly have been in part instinctive ; but having obtained one object by this means, he seems to set the same cause in action to gain another object, which he conceives to be similarly conditioned, and when, apparently by new observations (data), finding that this second object is confined by a force greater than that which he can command by his water-current, he desists immediately from his exertions, it is evident that three several mental processes have occurred, to wit : —

(*a*) The employment of like causes to produce like effects.

(*b*) The exercise of a certain amount of memory (individual education by experience).

(*c*) The correct estimation of the difference in force, exerted upon the cane, between the water-current of his making and the confining power of his mate's paw, e. g., judgment.

2° By the conditions of our definition, it would be necessary, in order that these acts of No. 2 might be instinctive, that the same should be observed of the majority of polar bears when similarly conditioned. An appeal to facts shows that these acts are rarely executed by bears. Hence it follows that the said acts of No. 2 were not, either in part or in whole, instinctive. Finally, by the conditions of the proposition, these acts, being neither reflex nor instinctive, must be rational, or else did not take place, e. g., either reason must exist in certain bears of the polar species, or the mind of man must refuse to think of the acts of said bears. The only attempt at the vitiation of the foregoing argument is conceived to exist in the fact that it rests upon but one observation.

JAMES P. MARSH.

The eccentricity theory of the glacial period.

Croll's eccentricity theory of the glacial period is certainly an attractive theory. The ingenuity and learning of its author have merited and received universal respect. The proposal thus to link together by one additional tie the sciences of astronomy and geology, is in harmony with that profound sense of the unity of nature, which is a dominant sentiment in modern science. In utter despair of the possibility of constructing any reliable time estimates by measuring the amount of erosion or deposition, every geologist would gladly welcome the opportunity of importing into his science something of the chronological definiteness which has been the boast of the astronomer. And it must, I think, be conceded that no very satisfactory explanation of a glacial period by means of purely terrestrial conditions has been proposed.

Nevertheless, there has always been a considerable degree of skepticism in regard to the fundamental conception of the eccentricity theory. The question whether the conditions of aphelion winter and perihelion summer, in an epoch of great eccentricity, would tend to accumulate snow and ice, and produce a glacial period in the hemisphere so conditioned, has never been so answered as to command universal assent. Indeed, J. J. Murphy has argued, with much plausibility, that the glaciated hemisphere would be the one with perihelion winter and aphelion summer.[1] Others have believed that there would be no appreciable effect in the direction of glaciation in either hemisphere. I desire to call attention to a class of well-known facts whose bearing upon the question has not, I think, been adequately regarded. A very brief preliminary discussion will suffice to show the bearing of the facts referred to.

There would evidently be two marked contrasts in the character of the seasons between the two hemispheres at an epoch of high eccentricity. The hemisphere with aphelion winter would have a long winter and a short summer, while the other hemisphere would have a short winter and a long summer. Again, the hemisphere with aphelion winter would have extremes of heat and cold, its summer being very hot and its winter very cold, while the climate of the other hemisphere would approximate a mean throughout the year. It is by no means certain that the effects of these two contrasts upon the matter of glaciation would be in the same direction. As regards the difference in the length of the seasons, I suppose there can be no doubt that increased length of winter would tend to glaciation. Other things being equal, the longer the winter, the larger would be the proportion of precipitation in the form of snow, and the smaller the proportion in the form of rain. And increased snow-fall would certainly tend to accumulation of snow and ice.

But what would be the effect of the difference in the intensity of the seasons? Would glaciation be favored by cold winters and hot summers, or by mild winters and mild summers — by a climate of extremes, or by a climate of means? It seems to me that a comparison of the northern and southern hemispheres at present, in the matter of glaciation, will suggest an answer to this question. The present value of the eccentricity of the earth's orbit is so small that its climatic effects are completely masked by geographical conditions. The northern hemisphere now has the perihelion winter, and the southern hemisphere the aphelion winter. So far, therefore, as astronomical conditions control climate, the northern hemisphere should have a climate of means, and the southern hemisphere of extremes. But this relation is completely reversed by geographical conditions. The great amount of land in the northern hemisphere gives that hemisphere a climate of extremes, while the vast expanse of water in the southern hemisphere produces a climate of means. This

[1] *Quarterly journ. of geolog. soc.*, xxv. 350, 1869 ; *Amer. journ. science*, [2] xlix. 115, 1870.

contrast will appear very striking to any one who will compare the maps of January and July isotherms, respectively, for the globe. The two maps will be seen to differ but slightly in the southern hemisphere, immensely in the northern. I know no reason why a contrast between extremes and means in climate, produced by geographical conditions, should have a materially different effect, as regards glaciation, from a like contrast produced by astronomical conditions. It appears, then, that a comparison of the northern and southern hemispheres may show us whether a climate of means or a climate of extremes is favorable to glaciation.

Now, there can be no doubt that at present the southern hemisphere is suffering a greater degree of glaciation than the northern. As the facts are so well known, it is only necessary to allude to them. New Zealand, with a mean temperature about the same as that of Switzerland, has glaciers extending as nearly to the sea-level as those of Norway.[1] Nor is this due to any exceptionally large snow-fall in New Zealand, for the precipitation there is no greater than in Norway, and considerably less than in Switzerland. Tierra del Fuega, with a mean temperature about equal to that of southern Norway, and with a winter temperature no colder than that of Switzerland, has glaciers extending to the sea.[2] The same is true of the island of South Georgia, if, indeed, perpetual snow does not descend to the level of the ocean (as reported by Captain Cook).[3]

It may, I think, fairly be concluded that glaciation depends less upon the coldness of the winter than upon the coolness of the summer. Not a climate of extremes, but a climate of means, tends to produce glaciation. It appears, accordingly, that the two characteristics of the seasons, in an epoch of high eccentricity would tend in precisely opposite directions, as regards glaciation. In one hemisphere, the length of the winter would tend to glaciation, while the intensity of extremes of temperature would oppose glaciation. In the other hemisphere, the shortness of the winter would oppose glaciation, while the approximation to a mean temperature would favor glaciation. The actual tendency to glaciation would be, then, the algebraic sum of two values of opposite signs. In which hemisphere would the tendency to glaciation predominate? And would the absolute value of the algebraic sum of the two tendencies in either hemisphere be sufficient to have any appreciable influence? I simply suggest these questions, making no attempt to answer them.

I may remark incidentally that there is something aparently unsound in the argumentation by which the advocates of the eccentricity theory seek to show that the hot perihelion summer would not melt the snow and ice. They virtually deny that the perihelion summer would be hot, urging that the temperature could not rise above the freezing-point until the ice was all melted.[4] It may well be conceded that the summer temperature could not rise much above the freezing point in the centre of a polar ice-cap, or at the apex of a snow-capped peak. But at the margin of a snow-field, polar or alpine, the climatic conditions would be very different. The ice-fields of a glacial period would not be created instantaneously in their maximum extent, but would be the results of a slow accumulation for many centuries. As each hemisphere in turn gradually approached the condition in which the climax of its winter would fall in aphelion, the snow-fields would be at first of very small extent. Outside the boundaries of those snow-fields, the land would be heated to a temperature increasingly hot, as year by year the climax of the summer approached the perihelion; and that high temperature of the surrounding areas would produce rapid melting at the margins of the snow-fields. Moreover, even at the extreme of glaciation, the area covered by ice would form but a small part of the surface of a hemisphere. Cold aphelion winters must be accompanied by perihelion summers not only potentially but actually hot.

WILLIAM NORTH RICE.

Wesleyan University, Middletown,
Conn., Aug. 16.

[1] *Science*, iv. 426, 1884.

[2] Darwin, 'Journ. of researches during voyage of H. M. S. Beagle,' p. 224. N. Y., 1875.

[3] Lyell, 'Principles of geology,' vol. i. p. 242. N. Y., 1872.

[4] Croll, 'Climate and time,' pp. 58-67. New York, 1875.

The causation of pneumonia.

In *Science* for Aug. 13, 1886, p. 133, I notice a paragraph relative to results of observations by Dr. Seibert of seven hundred and sixty-eight cases of pneumonia, wherein it appears that pneumonia prevails to its greatest extent "whenever there exists a low or falling temperature, with excessive and increasing humidity, and high winds." This reminds me that readers of *Science* may be interested to know that facts respecting a very much larger number of cases, and respecting pneumonia in different parts of the United States, in England, and in India, — that is to say, in several climates and under different conditions, — confirm to some extent the conclusions reached by Dr. Seibert, as mentioned by *Science*. Such statistics, presented by abstract at the last meeting of the American climatological association, demonstrate, I think, that the sickness from pneumonia is absolutely controlled by the temperature of the atmosphere. The higher the temperature, the less the sickness from pneumonia; and the lower the temperature, the more the sickness from pneumonia. This is equivalent to saying that that part of the conclusion of Dr. Seibert which relates to humidity is an error; because the absolute humidity of the atmosphere is, speaking roughly, inversely as its temperature, and there is most sickness from pneumonia when, or soon after, the air is driest absolutely; and there is least sickness from pneumonia when, or soon after, the air contains the most vapor of water, that is, when the temperature is highest. The error of many who have written on this subject, and probably the error of Dr. Seibert, consists partly in calling the 'per cent of saturation of the air' (technically known as 'the relative humidity,' the humidity of the atmosphere. But the curve for 'relative humidity' is not, when inverted, the same as the curve for pneumonia, as you may see by comparing such curves, on the diagrams I published, based upon over twenty-seven thousand weekly reports of sickness in Michigan, by observers in different parts of the state, and upon over one hundred and twenty thousand observations of the psychrometer during the same time, namely, the seven years, 1878-84. Relative humidity seems to have an opposite relation in the warm months to what it has in the cold months. The fact, which I think I have completely demonstrated, is, that pneumonia is quantitatively proportional to the coldness and dryness of the atmosphere;

and, as this is true for every month of the year, it follows that, if there is any pneumonia which is infectious, it is absolutely dependent upon those meteorological conditions for its action upon the human organism.

In the paper to which I have referred, I have advanced a theory of the causation of pneumonia consistent with the facts demonstrated; and, briefly outlined, it is as follows: Air expired from the human lungs is nearly saturated with vapor of water at a temperature of about 98° F., and this contains about 18.69 grains of vapor in each cubic foot. The quantity of vapor exhaled is at all times greater than the quantity inhaled; but when the air is very cold and dry, the quantity exhaled is excessive, as may be seen when we reflect that air at 32° F. can contain in each cubic foot only about two grains of vapor. The fluid which passes out from the blood into the air-cells of the lungs, and which normally keeps them moist, contains some of the salts of the blood; and the chloride of sodium, not being volatile, is mostly left in the air-cells when the vapor passes out with the expired air. When the air inhaled is excessively dry (as it always is when excessively cold), this salt collects in the air-cells of the lungs in considerable proportion. This is proved by my statistics, which show the increase of pneumonia at such times, taken in connection with the fact that chloride of sodium in the lungs is in excess in pneumonia, which was proved in 1851 by Lionel S. Beale, M.D., of London, England. Dr. Beale also verified the observations by Redtenbacher, made in 1850, that during the onward progress of pneumonia the chlorides disappear from the urine, and reappear when convalescence has been established. In the air-cells, the chlorides are irritating when they become concentrated; but the exudation of fibrine, which is the most prominent condition in pneumonia, is probably favored by a fact in osmosis which is not generally well understood, —namely, that albumen, which it is usually considered will not pass by osmosis, will pass through an animal membrane to a solution of chloride of sodium.

Thus the causation of pneumonia by the inhalation of cold dry air seems to be completely worked out. As a cause of deaths, pneumonia is one of the most important diseases. It is hoped that its prevention may now begin.

HENRY B. BAKER.

Lansing, Mich., Aug. 17.

The sweating sickness.

In Hume's 'History of England,' volume ii., p. 384, appears the following passage: "There raged at that time, in London and other parts of the kingdom, a species of malady unknown to any other age or nation, the 'sweating sickness,' which occasioned the sudden death of great multitudes, though it seemed not to be propagated by any contagious infection, but arose from the general disposition of the air and of the human body. In less than twenty-four hours the patient commonly died, or recovered; but when the pestilence had exerted its fury for a few weeks, it was observed, either from alterations in the air or from a more proper regimen which had been discovered, to be considerably abated."

The time of this endemic must have been about the summer of 1485, just a short time previous to the coronation of Henry VII. The historian makes no further mention as regards the nature of this malady; in fact is distressingly concise in his account of so interesting a disorder.

Now, the object of my letter is apparent: I wish a little more definite information concerning this so-called 'sweating sickness.' But if perchance, in my ignorance, I am inquiring about a disease the name of which is synonymous with one at present in existence, then the modern name will be all-sufficient.

E. W. EVANS.

Easton, Penn., Aug. 16.

[The 'sweating sickness' to which our correspondent refers prevailed in England during portions of both the fifteenth and sixteenth centuries; appearing for the first time in 1485, again in 1506, for the third time in 1517, and twice subsequently, in 1528 and 1551. During this last visit, it appeared in London July 7, and during the twenty-three days that it remained caused nearly a thousand deaths. The disease was in the nature of a fever, followed by sweating; commencing with pains throughout the body, flushes of heat, oppression at the stomach, and delirium, after which, a profuse perspiration of an offensive odor. Relapses were apt to occur, sometimes as many as twelve in number. Some regarded the disease as a rheumatic fever, others as a form of ague, and others still as an influenza. The first appearance of the disease, in 1485, was traced to the army that fought at Bosworth; the second, of 1517, occurred when London was crowded with foreign artisans; and that of 1528 was coincident with the great military operations of Francis I. in Italy. At the time the sweating sickness prevailed in England, that country was ravaged by diseases and pestilences of almost every name. Spotted fever, brain fever, epidemic flux, scurvy, diphtheria, small-pox, measles, scarlet fever, and erysipelas, — all figured largely as mortality factors during these two centuries.

That England was not blotted out of existence by pestilential disease during this epoch is a marvel. Houses were constructed without any regard to ventilation; the floors were made of loam covered with rushes, which were not removed, but were covered with others from time to time, until the deposit of twenty years and more had accumulated, — containing bones, broken victuals, and all manner of filth, and saturated with the discharges of man and beast. The streets were in the same condition, the filth being thrown into them from the houses. Of this condition of things Erasmus wrote, "If, even twenty years ago, I had entered into a chamber which had been uninhabited for some months, I was immediately seized with a fever." Add to this the gluttony and intemperance of the English people of this time, and some faint idea may be obtained of the influences at work to undermine the constitutions of our ancestors and prepare them for epidemic disease whenever it should appear. If our correspondent desires to study this disease in detail, he will find a full account in the following works: 'Historia regni Henrici, septimi regis Angliae, vol. ix. of the works of Francis Bacon; 'The epidemics of the middle ages,' J. F. C. Hecker, M. D., published by the Sydenham society; 'A boke or counseill against the disease commonly called the sweat or sweating sicknesse, made by Jhon Caius, doctour in physicke, 1552' (appendix to Hecker's 'epidemics of the middle ages'). A very admirable résumé of this epidemic disease, and of others, will be found in 'Public health,' by Wm. A. Grey, M. B., published by Henry Renshaw, London. — ED.]

SCIENCE.—SUPPLEMENT.

FRIDAY, AUGUST 27, 1886.

THE ORIGIN OF LANGUAGES, AND THE ANTIQUITY OF SPEAKING MAN.

IN the study of every science there arise from time to time difficult questions or problems which seem to bar the way of the student in one direction or another. It becomes apparent that on the proper solution of these problems the progress of the science mainly depends ; and the minds of all inquirers are bent earnestly on the discovery of this solution. Such, in biology, are the questions of the origin of life and the genesis of species. Anthropology, and its auxiliary or component sciences of comparative philology, ethnology, and archaeology, have their share of these problems. Among them, two of the most important are undoubtedly, in philology, the question of the origin of linguistic stocks, and in archaeology, the question of the epoch at which man acquired the faculty of speech. A brief consideration of these questions, in the light cast upon them by the most recent discoveries, may therefore be deemed to form an appropriate introduction to the work of our section.

The question of the origin of languages must be distinguished from the different and larger question of the origin of language, which belongs rather to anthropology proper than to the science of linguistics, and will come under consideration in the later part of our inquiry. Nor yet does our question concern the rise and development of the different tongues belonging to one linguistic stock or family, like the sixty languages of the Aryan or Indo-European stock, the twenty languages of the Hamito-Semitic family, the one hundred and sixty-eight languages enumerated by Mr. R. N. Cust as composing the great Bantu or South African family, and the thirty-five languages of the wide-spread Algonkin stock. Such idioms, however much they may differ, are in their nature only dialects. The manner in which these idioms originate is perfectly well understood. But we have no satisfactory theory to explain the distinction between the families themselves. When, for example, we have traced back the Aryan languages and the Semitic languages to their separate mother-tongues, which we are able to frame out of the scattered dialects, we find between these two mother-tongues a great gulf, which no explanation thus far proposed has sufficed to bridge over. How strongly the sense of this difficulty has been felt by the highest minds engaged in philological study, will be evident from two striking examples. Sixty years ago, Baron William von Humboldt found it (as Dr. Brinton states) "so contrary to the results of his prolonged and profound study of languages, to believe, for instance, that a tongue like the Sanscrit could ever be developed from one like the Chinese, that he frankly said that he would rather accept at once the doctrine of those who attribute the different idioms of men to an immediate revelation from God." Fifty years later Prof. Abel Hovelacque, in his work, 'La Linguistique,' declared, as the final conclusion of science, that there could be no conceivable community of origin between systems so unlike as that of the Indo-European and that of the Semitic tongues. "The abyss between the two systems," he affirms, "is not merely profound : it is impassable."

The number of distinct linguistic stocks is computed to exceed two hundred, most of which are found on the western continent. Various attempts have been made to explain their origin, but none have gained general acceptance. Some of the most eminent philologists have given up the question, in despair of a solution. Yet the simple and sufficient explanation has been lying close at hand, awaiting only, like many other discoveries in science, the observation of some facts of common occurrence to bring it to light. In the present case, the two observers who have made the conclusive facts known to us have both been Americans, and both of them writers of more than ordinary intelligence ; but both were entirely unknown in this branch of investigation, and both, moreover, had the ill-fortune of publishing their observations in works of such limited circulation that their important contributions to science have hitherto failed to attain the notice they deserved.

Before setting forth the facts, it will be well to state at once the result of the inquiry. Briefly, then, the plain conclusion to which all the observations point, with irresistible force, is that the origin of linguistic stocks is to be found in what may be termed the language-making instinct of very young children. From numerous cases, of which the history has been traced, it appears that,

Abstract of an address before the section of anthropology of the American association for the advancement of science at Buffalo, Aug. 19, 1886, by Horatio Hale, vice-president of the section.

when two children who are just beginning to speak are left much together, they sometimes invent a complete language, sufficient for all purposes of mutual intercourse, yet totally unintelligible to their parents and others about them. The first to observe, though not the first to publish, an instance of this nature was Miss E. H. Watson, a lady of Boston, the authoress of several esteemed works on historical subjects. In giving to the world, in 1878, a treatise by her father, the late George Watson, on 'The structure of language,' she prefixed to it an essay of her own on the 'Origin of language,' in which an interesting account is given of the 'childrens' language.' The children in question were twin boys, born in 1860, in a respectable family, residing in a suburb of Boston. They were constantly together, and an intense affection existed between them. "At the usual age," the authoress states, "these twins began to talk, but strange to say, *not* their 'mother-tongue.' They had a language of their own, and no pains could induce them to speak anything else. They persistently refused to utter a syllable of English. Their mother relates that although she could not understand their language, she contrived, by attention, to discover what they wished or meant." The important information is added that "even in that early stage, the language was complete and full; that is, it was all that was needed. The children were at no loss to express themselves in their plays, — their 'chatterings' with each other all day." At last they were sent to a school, where they gradually learned English, as children learn a foreign language, and the memory of their own speech faded from their minds.

Miss Watson, unfortunately, did not become aware of these circumstances until some time afterwards, when all recollection of this peculiar language was lost, except of a single word. Another observer, at about the same time, was more fortunate. A physician of Albany, Dr. E. R. Hun, in an article published in 1868, in the *Monthly journal of psychological medicine*, under the title of 'Singular development of language in a child,' has given a clear and scientific account of a similar phenomenon, with specimens of the language. In this case the speech was invented by a little girl, aged four years and a half, in conjunction with her brother, eighteen months younger than herself. About twenty of the words are given, most of which were used in several allied acceptations, — as *mea*, meaning both cat and furs; *migno-migno*, water, wash, bath; *bau*, soldier, music; *odo*, to send for, to go out, to take away; *waia-waiar*, black, darkness, a negro. The language had its own forms of construction, as in *mea waia-waiar*, 'dark furs' (literally, 'furs dark'), when the adjective follows its substantive. Dr. Hun adds, "She uses her language readily and freely, and when she is with her brother they converse with great rapidity and fluency."

Further inquiries have shown that such cases of child-language are by no means uncommon and these cases, it must be considered, are, after all, merely intensified forms of a phenomenon which is of constant recurrence. The inclination of very young children to employ words and forms of speech of their own is well known, though it is only under peculiar circumstances that this language acquires the extent and the permanence which it attained in the cases now recorded.

In the light of the facts which have now been set forth, it becomes evident that, to insure the creation of a speech which shall be the parent of a new linguistic stock, all that is needed is that two or more young children should be placed by themselves in a condition where they will be entirely, or in a large degree, free from the presence and influence of their elders. They must, of course, continue in this condition long enough to grow up, to form a household, and to have descendants to whom they can communicate their new speech. We have only to inquire under what circumstances an occurrence of this nature can be expected to take place.

There was once a time when no beings endowed with articulate speech existed on the globe. When such beings appeared, the spread of this human population over the earth would necessarily be gradual. So very slow and gradual, indeed, has it been, that many outlying tracts — Iceland, Madeira, the Azores, the Mauritius, St. Helena, the Falkland Islands, Bounty Island, and others — have only been peopled within recent historical times, and some of them during the present century. This diffusion of population would take place in various ways, and under many different impulses; — sometimes as the natural result of increase and overcrowding, sometimes through the dispersion caused by war, frequently from a spirit of adventure, and occasionally by accident, as when a canoe was drifted on an unknown shore. In most instances, a considerable party, comprising many families, would emigrate together. Such a party would carry their language with them; and the change of speech which their isolation would produce would be merely a dialectical difference, such as distinguishes the Greek from the Sanscrit, or the Ethiopic from the Arabic. The basis of the language would remain the same. No length of time, so far as can be inferred from the present state of our knowledge, would suffice

to disguise the resemblance indicating the common origin of such dialect-languages. But there is another mode in which the spread of population might take place, that would lead in this respect to a very different result. If a single pair, man and wife, should wander off into an uninhabited region, and there, after a few years, both perish, leaving a family of young children to grow up by themselves and frame their own speech, the facts which have been adduced will show that this speech might, and probably would, be an entirely novel language. Its inflections would certainly be different from those of the parent tongue, because the speech of children under five years of age has commonly no inflections. The great mass of vocables, also, would probably be new. The strong language-making instinct of the younger children would be sufficient to overpower any feeble memory which their older companions might retain of the parental idiom. The baby-talk, the 'children's language,' would become the mother-tongue of the new community, and of the nation that would spring from it.

Those who are familiar with the habits of the hunting tribes of America know how common it is for single families to wander off from the main band in this manner, — sometimes following the game, sometimes exiled for offences against the tribal law, sometimes impelled by the all-powerful passion of love, when the man and woman belong to families or clans at deadly feud, or forbidden to intermarry. In these latter cases, the object of the fugitives would be to place as wide a space as possible between themselves and their irate kindred. In modern times, when the whole country is occupied, their flight would merely carry them into the territory of another tribe, among whom, if well received, they would quickly be absorbed. But in the primitive period, when a vast uninhabited region stretched before them, it would be easy for them to find some sheltered nook or fruitful valley, in which they might hope to remain secure, and rear their young brood unmolested by human neighbors.

If, under such circumstances, disease or the casualties of a hunter's life should carry off the parents, the survival of the children would, it is evident, depend mainly upon the nature of the climate and the ease with which food could be procured at all seasons of the year. In ancient Europe, after the present climatal conditions were established, it is doubtful if a family of children under ten years of age could have lived through a single winter. We are not, therefore, surprised to find that no more than four or five linguistic stocks are represented in Europe, and that most of these are believed to have been of comparatively late introduction. In California, on the other hand, where the climate is mild and equable beyond example, and where small fruits, roots, and other esculents, abound at all seasons of the year, the aborigines are found to speak languages belonging to no less than nineteen distinct stocks. In Brazil, where the same conditions prevail, more than a hundred stocks, lexically distinct, have been found to exist. A review of other linguistic provinces yields results which strongly confirm the views now presented. A curious ethnological fact which tends in the same direction is the circumstance, which has been noticed by Major Powell, that, as a general thing, each linguistic family has its own mythology. Of course, when the childish pair or group, in their isolated abode, framed their new language and transmitted it to their descendants, they must necessarily at the same time have framed a new religion for themselves and their posterity; for the religious instinct, like the language-making faculty, is a part of the mental outfit of the human race.

But we are now brought face to face with another problem of great difficulty. The view which has just been presented shows that all the vast variety of languages on earth may have arisen within a comparatively brief period; and many facts seem to show that the peopling of the globe by the present nations and tribes of men is a quite recent event. The traditions of the natives of America, North and South, have been gathered and studied of late years, by scientific inquirers, with great care and valuable results. All these traditions, Eskimo, Algonkin, Iroquois, Choctaw, Mexican, Maya, Chibcha, Peruvian, represent the people who preserved them as new-comers in the regions in which they were found by the whites. Ethnologists are aware that there is not a tradition, a monument, or a relic of any kind, on this continent, which requires us to carry back the history of any of its aboriginal tribes, of the existing race, for a period of three thousand years. In the Pacific Islands the recent investigations have had a still more striking and definite result. We know, on sufficiently clear evidence, the times when most of the groups, from New Zealand to the Sandwich Islands, were first settled by their Polynesian occupants. None of the dates go back beyond the Christian era. Some of them come down to the last century. In Australia, the able missionary investigators have ascertained that the natives had a distinct tradition of the arrival of their ancestors, who entered by the north-west coast. It is most unlikely that, among such a barbarous and wandering race, a tradition of this nature should be more than two thousand years old. Probably it is much less ancient. We know

positively that the neighboring group of New Zealand was settled only about five hundred years ago. Passing on to the old continent, we find that the Japanese historical traditions go back, and that doubtfully, only to a period about twenty-five hundred years ago; those of China only about four thousand years; those of the Aryans, vaguely, to about the same time; the Assyrians, more surely, a little longer; and the Egyptians to the date fixed by Lepsius for Menes, not quite four thousand years before Christ. No evidence of tradition, or of any monument of social man, points to his existence on the earth at a period exceeding seven thousand years before the present time. Yet the investigations which have followed the discoveries of Boucher de Perthes have satisfied the great majority of scientific men that human beings have been living on the globe for a term which must be computed, not by thousands of years, but by tens and probably hundreds of thousands. Writers of all creeds, and of all opinions on other subjects, concur in the view that the existence of man goes back to a remote period, in comparison with which the monuments of Egypt are but of yesterday; and yet these monuments, as has been said, are the oldest constructions of social man which are known to exist. How shall we explain this surprising discrepancy? How shall we account for the fact that man has existed for possibly two hundred thousand years, and has only begun to form societies and to build cities within less than seven thousand years? In other words, how, as scientific men, shall we bring the conclusions of geology and palaeontology into harmony with those of archaeology and history?

Fortunately, the geologists and physiologists themselves, by their latest discoveries, have furnished the means of clearing up the perplexities which their earlier researches had occasioned. We learn from these discoveries that while a being entitled to the name of man has occupied some portions of the earth during a vast space of time, in one and perhaps two geological eras, the acquisition by this being of the power of speech is in all probability an event of recent occurrence. The main facts on which this opinion is based must necessarily, in this summary, be very briefly stated.

The earliest men of whom we have any certain knowledge, the paloeolithic men, as they are styled, are distinguished by scientific investigators, as is well known, into two distinct races, belonging to widely different epochs. Prof. Boyd Dawkins styles the earlier race the 'river-drift men,' and the later the 'cave-men.' The river-drift men were, in his view, hunters and savages of the lowest grade. In his opinion, this race is now "as completely extinct as the woolly rhinoceros or the cave-bear." We have, he considers, no clue to its ethnology; and its relation to the race that succeeded it is doubtful. The cave-men were of a much higher order, and were especially remarkable for their artistic talents. Prof. de Quatrefages distinguishes the types of the two races as the 'man of Canstadt' and the 'man of Cro-Magnon,'—terms derived from places where crania belonging to these races have been found. Prof. A. de Mortillet knows the earlier race as the 'Chellean man' or the 'man of Neanderthal,' and the later as the 'Magdaleoran man,'—designations also derived from localities where their remains or their implements have been discovered. An under-jaw of an individual of this race, the celebrated 'jawbone of La Naulette,' affords what Prof. de Mortillet considers decisive evidence that its possessor had not the faculty of speech. This evidence is thus stated by him: "In the middle of the inner curve of the jaw, in place of a little excrescence called the 'genial tubercle,' there is a hollow, as with monkeys. Speech or articulate language," he continues, "is produced by movements of the tongue in certain ways. These movements are effected mainly by the action of the muscle inserted in the genial tubercle. The existence of this tubercle is therefore essential to the possession of language. Animals which have not the power of speech do not possess the genial tubercle. If, then, this tubercle is lacking in the Naulette jawbone, it is because the man of Neanderthal, the 'Chellean man,' was incapable of articulate speech."

In 1880, another jawbone belonging to this race was found by Prof. Maschka in the Schipka cave, in north-eastern Moravia; and in this jaw, also, the 'genial tubercle' was lacking. The inference derived from this evidence is strengthened by the peculiar shape of the crania belonging to this race, which are singularly low in the frontal region, leading to the belief that the third or lower frontal convolution of the brain, sometimes called 'Broca's convolution,' was imperfectly developed in the men of this race, as it is known to be in the anthropoid apes. It is in this convolution that Dr. Paul Broca has determined the seat of the faculty of language, Any lesion or disease of this part of the brain, as medical men are aware, produces aphasia, or the loss of the power of speech.

The succeeding race, the cave-men, or men of Cro-Magnon, possessed, as their osseous remains show, not only the 'genial tubercle,' but remarkably high and well-developed crania. Prof. de Quatrefages pronounces them 'a magnificent race.' Their carved and engraved implements display a superior artistic faculty. In the opinion

of Dr. Broca, they were 'on the threshold of civilization.' They seem to have been contemporaries and perhaps offshoots of the highly endowed populations of early Egypt and Assyria. These singularly gifted populations of north-eastern Africa, south-western Asia, and western Europe were, so far as can be judged from the existing evidence, the earliest representatives of speaking man on the globe. Yet there can be no doubt that they were descended from the river-drift race. We have not here to deal with the origin of a new species, but simply with that of a variety. That in some family of the primitive speechless race two or more children should have been born with the faculty and organs of speech is in itself a fact not specially remarkable. Much greater differences between parents and offspring frequently appear. Among these, for example, is one so common as to have received in physiology the scientific name of polydactylism, — a term applied to the case of children born with more than the normal number of fingers. M. de Quatrefages mentions that in the family of Zerah Colburn, the celebrated calculator, four generations possessed this peculiarity, which commenced with Zerah's grandfather. In the fourth generation four children out of eight still had the supernumerary fingers, although in each generation the many-fingered parent had married a person having normal hands. Plainly, he adds, if this Colburn family had been dealt with like the Ancon breed of sheep, a six-fingered variety of the human race would have been formed; and this, it may be added, would have been a far greater variation than was the production of a speaking race descending from a speechless pair. The appearance of a sixth finger requires new bones, muscles, and tendons, with additional nerves leading ultimately to the brain. There is good reason to believe that the first endowment of speech demanded far less change than this.

Many skilled observers have sought to discover by various indications, such as the accumulation of débris in caves, the layers of earth formed by streams, the growth of bogs, and other evidences, the time which has elapsed from the era of the cave-men and the neolithic race to our own time. All their conclusions are in substantial accord. While the existence of the earlier race, the river-drift race, goes back to an indefinite period, which, according to some opinions, may exceed two hundred thousand years, nearly all the estimates place the appearance of the neolithic race, or men of the polished-stone epoch, within seven thousand years, and that of their predecessors, the cave-men, within eight thousand years, from our own time.

The question of the region in which speaking man first appeared is one on which there is room for a wide difference of opinion. It is a question about which no one will venture to dogmatize. The natural supposition, of course, would be that this first appearance took place somewhere near the centres of the earliest civilization. These centres were in Egypt and Assyria. Between those countries lies Arabia, in which, amidst the sandy desert that protects the land from invasion, there are many oases, large and small, blessed with a most genial climate and a fruitful soil. From that primitive centre, if such it was, the increasing population would speedily overflow into the plains of Mesopotamia and the fertile valley of the Nile; and there, or in their near vicinity, nearly all the animals which were first tamed, and nearly all the plants which were first cultivated, would be found. We need not be surprised, therefore, to find that the great majority of investigators have looked to south-western Asia for the primitive seat of the human race. The most distinct tradition that has come down to us of the earliest belief respecting the creation of man — the tradition which is preserved in the Hebrew narrative — places it in an oasis on the Arabian border, and dates it apparently at about the time when, as all the evidence seems to show, man endowed with speech first appeared.

The conclusions to which this inquiry, guided by the most recent discoveries of science, has directed us, may be briefly summed up. We find that the ideas of the antiquity of man which have prevailed of late years, and more especially since Lyell published his notable work on the subject, must be considerably modified. No doubt, if we are willing to give the name of man to a half-brutish being, incapable of speech, whose only human accomplishments were those of using fire and of making a single clumsy stone implement, we must allow to this being an existence of vast and as yet undefined duration, shared with the mammoth, the woolly rhinoceros, and other extinct animals. But if, with many writers, we term the beings of this race the precursors of man, and restrict the name of men to the members of the speaking race that followed them, then the first appearance of man, properly so styled, must be dated at about the time to which it was ascribed before the discoveries of Boucher de Perthes had startled the civilized world, — that is, somewhere between six thousand and ten thousand years ago. And this man who thus appeared was not a being of feeble powers, a dull-witted savage, on the mental level of the degenerate Australian or Hottentot of our day. He possessed and manifested, from the first, intel-

lectual faculties of the highest order, such as none of his descendants have surpassed. His speech, we may be sure, was not a mere mumble of disjointed sounds, framed of interjections and of imitations of the cries of beasts and birds. It was, like every language now spoken anywhere on earth by any tribe, however rude or savage, a full, expressive, well-organized speech, complete in all its parts. The first men spoke, because they possessed, along with the vocal organs, the cerebral faculty of speech. As Professor Max Müller has well said, "that faculty was an instinct of the mind, as irresistible as any other instinct." It was as impossible for the first child endowed with this faculty not to speak, in the presence of a companion similarly endowed, as it would be for a nightingale or a thrush not to carol to its mate. The same faculty creates the same necessity in our days; and its exercise by young children, when accidentally isolated from the teachings and influence of grown companions, will readily account for the existence of all the diversities of speech on our globe.

WHAT IS NERVE-FORCE?

A DISTINGUISHED biologist has remarked, with great truth, that the study of the nervous system is the true field of battle for physiologists, all other investigations, however interesting and important, being of the nature of skirmishes, preparatory for and surely leading up to the final conflict, in which we must engage before we can hope to gain a position from which nature's most mysterious processes are laid bare to our view. Of all the functions of the nervous system, the one which, at first sight, would seem most accessible to investigation, is that of the nerve-fibre itself. What conception can we form of the physical or chemical changes which take place in those white glistening bands which are for us the only channels through which knowledge of the physical universe can be obtained, and which also enable us to impress upon the world around us the evidence of our conscious personality?

With the discoveries of Du Bois Reymond, the hope arose that nerve-activity might be explained as an electrical phenomenon, and the attempts made to build up a satisfactory electrical theory of nervous action have been numerous and ingenious. The important facts which forbid the identification of nerve-force with electricity are: the absence of an insulating sheath on the nerve-fibre, the slow rate at which the nerve-force is transmitted, and the effect of a ligature on a nerve in preventing the passage of nerve-force, while not interfering with that of electricity. The electrical phenomena connected with the functional activity of nerves (action-current, electrotonus) appear, therefore, to be secondary in their character, and not to constitute the essential process in nerve action. In this connection should be noted an experiment of d'Arsonval,[1] which shows how the electrical phenomena associated with the activity of nerves may be imitated by purely physical means. This observer filled a glass tube, of one or two millimetres interior diameter, with drops of mercury alternating with drops of acidulated water, thus forming a series of capillary electrometers. The tube was closed at its two ends with rubber membranes, and was provided with lateral openings by which its interior could be connected with electrical conductors. A blow upon one of the membranes caused an undulation of the liquid column, which was propagated from one end to the other of the tube, and was accompanied by a wave of electrical oscillation, which was propagated at the same rate. The phenomenon is, according to d'Arsonval, to be explained as follows: The blow upon the membrane changes the form of the surface of contact between the first two cylinders of mercury and acidulated water. This change of form is transmitted to the following cylinders with a rapidity dependent upon the nature of the fluid. But each of these changes of shape is accompanied by the production of an electric current (Lippmann's phenomenon, due to variation of superficial tension), and the tube is therefore traversed by an electric wave, which necessarily has the same rate as the undulation of the liquid column. The analogy between this phenomenon and the wave-like propagation of the action-current in nerves is sufficiently obvious.

In studying the nature of nerve-force, two alternatives present themselves. We may conceive the impulse to be conducted through the nerve-fibre by a series of retrograde chemical changes in the successive molecules of the nerve-substance, the change occurring in one portion of the fibre acting to produce a similar change in the neighboring portion. As this process is associated with the using up of organic material, and the consequent discharge of potential energy in the successive portions of the nerve, the theory may be called 'the discharging hypothesis.' The burning of a line of gunpowder may be taken as an example of this sort of action. On the other hand, we may conceive that the nerve-force is transmitted from molecule to molecule by some

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[1] *Comptes rendus soc. biol.*, April 3, 1886.

sort of vibratory action, as sound is transmitted through a stretched wire. As this theory does not involve the using up of any material, but simply the transferring of motion, it may be called 'the kinetic hypothesis.'

Inasmuch as the discharging hypothesis involves the destruction of organic material, we may, if this theory be correct, reasonably expect to find in the active nerve-fibre evidences of chemical decomposition and of heat production. Moreover, if the organic substances are used faster than they are replaced, or their products of decomposition removed, as they would naturally be under constant stimulation, we may expect to observe a diminution of nerve-action during the continuance of the stimulation; in other words, we shall have the phenomena of fatigue. On the kinetic hypothesis, on the other hand, we may expect to find an entire absence of chemical decomposition and fatigue, and, if the moving particles are endowed with perfect elasticity, an absence also of heat production.

The only functional chemical change of nerves for the existence of which an experimental proof has been offered, is the change in the reaction with test-paper. Just as the normally alkaline tissue of muscles becomes neutral or acid in activity, so, according to Funke[1] and Ranke,[2] do nerve-fibres and the white substance of the spinal cord change in activity from an alkaline to an acid reaction. Liebreich[3] and Heidenhain,[4] on the other hand, experimenting with a slightly different method, failed to get any evidence of the acidification of nerves in connection with functional activity. The phenomenon must indeed be a delicate one, since Ranke himself urges that the question should be decided by experiments on the spinal cord, and should not depend upon the 'doubtful results of tests applied to the nerve-trunks.' Now, since the cord contains gray as well as white substance, and as the gray substance, according to Ranke himself, becomes more acid than the white in functional activity, it is clear that an acid reaction of the white substance of the spinal cord may depend upon an acid formed in the gray and passing by diffusion into the white substance. This possibility, which is indeed admitted by Ranke, seems to deprive the experiments on the spinal cord of what little value they possessed as evidence of the production of acid in connection with the activity of nerve-fibres.

The other chemical changes which have occasionally been asserted to occur in active nerves, rest on still weaker experimental evidence, and it is therefore clear that chemical investigation gives us but little reason for maintaining a discharging, in opposition to a kinetic, theory of nerve action.

The first experiments to test the heat-production of active nerves were those of Helmholtz,[1] who, after studying the analogous phenomenon in muscles, extended his investigations to nerve-fibres. He failed, however, when all sources of error were carfully avoided, to obtain any evidence of heat-production in connection with nervous activity, though his apparatus was capable of registering a change of temperature of 0.002° C. Similar negative results were obtained by Heidenhain.[2] On the other hand, Valentine,[3] Oehl,[4] and Schiff[5] maintained that nerve-fibres really are warmed by the passage of the nerve impulse. It seems, then, that the results of thermometric investigations speak no more positively than those of chemical research in favor of a discharging rather than a kinetic theory of nerve action.

The evidence of the activity of a nerve may be either direct or indirect. The direct evidence consists in the occurrence of that change of the electrical condition known as the 'negative variation,' of Du Bois Reymond, or the 'action-current,' of Hermann. The latter writer quotes the former as authority for the statement that this phenomenon becomes less intense in successive repetitions of the experiment, and regards this as evidence of the exhaustion of the nerve-fibre. Unfortunately, Hermann does not refer to the exact passage which contains this statement, and an examination of the chapter on the negative variation of nerves, in Du Bois Reymond's 'Untersuchungen,' fails to show any systematic study of the effects of fatigue on this phenomenon.

The indirect evidence of the activity of a nerve consists in the effect which it produces upon the central and peripheral organs with which it is connected. Of these effects, the contraction of a muscle is the one which is most conveniently observed, but the fact that a muscle is more readily exhausted than a nerve, renders it impossible to study the fatigue of nerves in this way without some special modification of the experiment.

Bernstein[6] was the first to employ the muscular contraction in experiments on the exhaustion of nerves. This observer finally reached the conclusion that a nerve may be exhausted by 5′–15′ tetanic stimulation. The experiments of Bern-

[1] *Arch. anat. und phys.*, 1859, 835.

[2] *Centralbl. med. wiss.*, 1868, 769; 1869, 97.

[3] *Tagebl. naturf. vers. Frankfurt*, 1867, 73.

[4] *Studien*, iv. 248; *Centralbl. med. wiss.*, 1868, 833.

[1] *Archiv. anat. und phys.*, 1848, 158.

[2] *Studien*, iv. 250. [3] Moleschott 'Undersuch.' ix. 225.

[4] *Gaz. med. Paris*, 1886, 225. [5] *Pflüger's archiv.*, iv. 230.

[6] *Pflüger's archiv.*, xv. 298.

stein have recently been repeated by Wedenskii,[1] who was unable to find any evidence of the exhaustion of the nerve, even after the tetanic stimulation had continued six hours. A study of the subject upon warm-blooded animals seeming desirable, experiments were made upon cats, in the laboratory of the Harvard medical school.[2] It was found that stimulation of the nerve lasting from one and a half to four hours (the muscle being prevented from contracting by curare) did not exhaust the nerve, since on the elimination of the curare the muscle began to contract.

It thus appears that evidence of fatigue in nerves resulting from functional activity is as difficult to obtain as that of chemical change or of heat-production. It is conceivable that the irritability of a nerve should depend upon its possessing a certain definite chemical composition, constantly maintained by metabolic changes, and yet that the irritation of the nerve should produce no change whatever in its composition.

In support of this view, an analogy may be drawn from the physiology of the muscular system. We find here that the power of the muscles to perform their function is intimately associated with the amount of nitrogenous material undergoing decomposition in the body, but the performance of a given amount of muscular work, if within physiological limits, does not effect the amount of nitrogen excreted. In the case of muscles, to be sure, we have evidence of a considerable decomposition of non-nitrogenous material, and also of heat-production in connection with functional activity, but, if we limit our consideration to the nitrogenous element of muscular substance, the hypothesis above proposed for nerves finds its complete analogy in the muscular system.

We have thus seen that investigations into the chemical changes, the heat-production, and the fatigue of active nerves, all lead to results more favorable to a kinetic than to a discharging theory of nerve action.

We may, therefore, reasonably hope that future researches, if directed on this line, will throw further light on this most mysterious and interesting process

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IN the 'Catalogue of printed books' in the British museum, now issuing in random instalments, one heading which has just been completed — 'Academies' — is of special scientific interest. This and 'Periodical publications' (which is also nearly completed, four of the five parts being out) will indeed include reference to a large proportion of scientific literature, and it is not probable that any library in the world can at all compete with the British museum in its general completeness in these departments. The earlier publication of this list of titles of society publications would have rendered the catalogues of Scudder and Bolton more satisfactory. The volume of 'Academies' is a folio of 1018 pages. London alone occupies one part with nearly 200 pages, though Paris has less than 90. The publications are arranged under the name of the issuing body, and these alphabetically under the town where situated, the towns having their English form and making a single alphabet. Thus Compiegne, Concord, Constantina, Constantinople, and Copenhagen follow in that order. A few countries are introduced into the alphabet for some general societies, though other societies with equal right to a national name are placed under the seat of government. The United States does not appear, and it would be difficult to say where to look for our peripatetic societies. Certainly the American association publications can nowhere be found, though they are doubtless in the museum, as we note one or two other omissions known to us to be there. Only completed series are fully entered; of others, the first volume in the possession of the museum is given, with the added words, 'in progress.' There is no transliteration, but Greek, Russian, Persian, or what not, are mixed in one alphabet with the Roman. Some curious rules have been followed in the alphabetization: thus 'Société cuvierienne' precedes 'Société d'acclimatation,' because of the preposition in the latter; yet Le 'bureau des longitudes' is made to precede La 'société cuvierienne' by dropping the objectionable particles from the full names. These, of course, are minor matters, and it would take a good many such to detract in any serious measure from the value of this excellent and carefully edited work.

— Messrs. Jackman and Webster report in the *Photographic news* their results in photographing the retina of the human eye. A small camera was employed, placed behind an ophthalmoscope, and the albo-carbon gaslight was the means of illumination. In the photograph the normal cupping of the optic disk and the principal blood-vessels are readily discerned. It is evident that but a beginning has been made in this method of research; but, if continued, very valuable results may be obtained. The method of Brainerd and French in photographing the vocal cords and the interior of the larynx promises equally well, and is now employed by a number of laryngologists in making permanent records of abnormalities in these parts.

[1] *Centralbl. med. Wiss.*, 1884, 65.

[2] Bowditch, *Journal of physiology*, vi. 133.